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CONTENTS

	Page.
I. Proceedings—February meeting.	
II. On the Efficiency of Porous Porcelain Filters, by Lucius Pitkin, Ph.B.	22
III. On the Solubility of Alumina Residues from Baking Powders in Gastric Juice, by Lucius Pitkin, Ph.B.	27
Abstracts from Chemical Journals:	
General and Inorganic Chemistry.	33
Organic Chemistry	34
Analytical Chemistry	35
Industrial Chemistry	38
Abstracts of American Patents relating to Chemistry	39

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A. A. BRENEMAN, 97 Water St., New York.

REGULAR MEETING, Friday, February 4th, 1887.

Vice-President T. Sterry Hunt in the chair.

The minutes of the preceding meeting and those of the meeting of the Board of Directors held January 7th were read and accepted.

The following were elected regular members: C. J. Roney, A. H. Sabin and Wm. K. Newton, M. D.

Dr. Hunt read a paper on "The Law of Volumes in Chemistry."

Mr. Lucius Pitkin read papers on the "Efficiency of Porous Porcelain Filters" and on the "Solubility of Alumina Residues from Baking Powders in Gastric Juice," which were followed by some discussion.

The meeting was then adjourned.

T. D. O'CONNOR,
Recording Secretary.

ON THE EFFICIENCY OF POROUS PORCELAIN FILTERS.

LUCIUS PITKIN, PH. B.

The purification of water supplies by filtration is by no means a latter day invention. It is mentioned by Pliny as used in his time. Avicenna refers to it, and both of these authors speak of wool as the filtering agent. The variety of filtration apparatus, however, which we discuss to-night can claim a still higher antiquity in its essential features, for the ancient Egyptians used, not indeed porous porcelain, but the nearest approach possible to it, porous pottery, to improve the turbid waters of the Nile.

This was certainly before the germ theory of disease had been suggested, although a recent writer on the "Malarial Germ" notices that Lucretius—95 B.C.—had ascribed the fever of the Pontine Marshes to a micro-organism. Within the last fifty years about everything new in science has been hailed as a resurrection of the ideas of Lucretius, whether rightly or wrongly we will not here consider, but simply remark that the object in view in old systems of filtration was probably clarification alone. Since the proof, however, that some diseases are caused by micro-organisms and the possibility that these micro-organisms may obtain access to water supplies, and hence to the human system, the public have demanded more of a filter than simply a power of rendering turbid water bright. I have elsewhere said: "A filter is essentially a sieve, and the organisms that it must stop in their passage are in the neighborhood of $\frac{1}{25000}$ of an inch diameter. A filtering material sufficiently close grained, or tightly enough packed to prohibit their passage, must necessarily deliver water somewhat slowly in comparison to filtering surface. The fallacy of employing a little metal sphere a couple of inches in diameter, loosely packed with sponge or charcoal and screwed to a faucet, the water of which it will deliver almost as fast as it will flow is therefore apparent, and yet

such filters have been and probably are now sold by the thousand.

If they claimed, as is their proper function, to catch simply the larger suspended matter and deliver the water freer from floating debris, we should have nothing to say against them ; it would be at once seen that they were no better than the primitive plan of tying a small flannel bag to the faucet, which, indeed, is better since the bag is more readily cleaned than most of such filters when becoming clogged, and a prime essential of all filtration is that the filter should be kept clean since, if it is not, it may foul rather than purify the water."

The substances which have proven most efficacious in filtering, especially on a small scale, are all very close grained, as artificial porous stone, porous porcelain, compressed molded carbon, etc. To these I must add asbestos fibre, prepared as mentioned in this journal, Vol. 8, page 192.

I have not personally experimented with this micro-membrane, but the figures given by Mr. Stebbins place it high in the list. We are here especially speaking of what can be designated as permanent filtering strata, as opposed to temporary ones requiring frequent renewal.

The experiments of Dr. Frankland and others have shown that almost all granular bodies, as sand, powdered coke, etc., will, in strata of a few inches in depth, act as efficient filtering agents.

This power is, however, only temporary, as the shifting nature of the material allows the germs gradually to work through. Aside from the question of efficiency attention must be called to the advantage that a rigid permanent material possesses over one needing frequent renewal, in the matter of the attention required in refilling, etc. While some forms of filtration requiring renewal are well adapted to filtering on the enormous scale required for city supplies, I think I am justified in recommending the use of rigid porous materials as best for domestic filtration on a small scale, provided of course that the material is sufficiently close grained. Porous porcelain seems to possess exceptional advantages for filter construction since, from changes in its composition and in the mechanical condition of its constituents, it can be made of different densities and closeness of grain, while before baking it can be made into any shape that may be required. A form which has been re-

cently introduced into this country consists of a tube of the unglazed porcelain of 1″ external diameter and 8″ length. When but a single tube is used this is surrounded by a cylinder of brass, glazed porcelain or any suitable substance with a bore sufficiently wide to allow an annular space of about ¼ inch between the two walls. It is easily seen that we have here a high proportion between the filtering surface and the size of the apparatus. The outer shell communicates directly with the faucet, which makes the filtration proceed from the outside of the tube to the interior, and thus allows of a more easy cleaning than where the direction of the current is in the reverse order. The walls of the tube being vertical also allows of much of the debris falling to the bottom, and thus aids in keeping the upper part of the tube in a condition for rapid filtration.

When it is desired to filter more water than the capacity of a single tube will permit of we have only to increase the diameter of the outer shell, and put within it from 3 to 100 tubes instead of one.

A screw clamp with a washer confines the porcelain tube within the shell, so arranged however that the nozzle-shaped lower end of the tube, through which the filtered water runs, may protrude. It was on such a form of filtration apparatus that my experiments were made. The tests were made upon two varieties of tubes (that is, tubes made with porcelain of two different densities). The denser marked "B," the lighter "F," the walls of the filtering tube being of ¼″ thickness. My attention was first called to this form of apparatus by the "editor" of one of our monthlies, who was desirous of knowing whether the claims made in its behalf could be substantiated. The tubes tested were therefore picked by myself at random from the stock, and it is probable represent fairly the character of the article now offered the public.

The tests were made in the usual way now employed in biological water analysis, *i. e.*, cultivation in sterilized nutrient gelatine on sterilized plates, with all precautions taken to secure freedom from accidental contamination during the sowing and subsequent incubation. The nutrient gelatine was prepared mainly according to the formula given by Frankland, but in place of the one gramme of salt which he recommends 5 grammes were used to the litre.

When used in this proportion the culture medium seems better suited to germ growth than where less salt is employed. The tests were varied and some purposely made very severe, to determine not only their efficiency under ordinary conditions, but also if possible the limits of their action.

In one set of experiments water purposely polluted, and containing 24000 germs to a c.c. was employed. In another set tank water with a variable number of bacteria—1400 to 3800 per c.c. —was used.

The first series of tests were made upon a "B" filter and an "F" filter; the water, as before stated, was foul with sewage and contained an enormous number of germs. The impure water was kept filtering under a head of two feet for 8 hours per day; after using one day the water escaping from the filters was tested biologically with the following results:

Filter "B"	0 germs per c.cm.
Filter "F"	0 " " "
Unfiltered water	24000 " " "

After 21 days use—8 hours in 24—another series of determinations were made:

Filter "B"	0 germs per c.cm.
Filter "F"	1560 " " "
Unfiltered water	24000 " " "
Efficiency—"B"	100 per cent.
Efficiency—"F"	93 "

Under exceptionally severe testing, then, we see one of the filters exhibiting perfect filtration after 3 weeks running on diluted sewage, while the other is allowing about one germ in 15 to pass. There is, however, no necessity for leaving the filter so long without cleaning, as it can so easily be removed, cleaned and replaced. The second series of tests were made with tank water containing about $\frac{1}{10}$ as many bacteria as the fluid before used, and representing more nearly what the filter would be required to cope with in actual practice. For this series I employed four other "B" filters and a different "F" filter. They were all set up with a common water supply on October 13th, 1886. The head of water was four feet,

and to expedite the process the water was allowed to filter day and night without interruption. The object of this series was to determine how many hours running under such conditions each filter would stand without recleansing, and still deliver germ-free water. Sowings were made on October 18th, after 5 days (120 hours), and all the four "B" filters and the "F" filter were delivering water absolutely germ free. Tests at the end of 10 days showed *two* of the "B's" and the "F" filter delivering water free from germs. While at the end of 15 days, 360 hours, *one* of the "B" filters and the "F" filter were giving perfect results. It would seem wise, therefore, to clean the filters once in about 5 days, since when germs commence to pass through in appreciable quantity, they soon cause the effluent water to be as rich in germ life as the unfiltered fluid. In this connection I may perhaps be allowed to state a theory, possibly not new, but which I have not as yet seen advanced in reference to the filtering process in rigid porous substance. We have said that a filter is essentially a sieve, and so it is but with an important difference. An ordinary sieve of say 50 mesh will not allow powder of larger diameter than its holes to pass, and would not after a year's shaking—granted the powder has not been broken—any more readily than at the first moment. How is it that a filter of porous material will deliver water absolutely free from germs when used and cleaned properly, and yet with neglect will let the water pass with a minimum of improvement. With a shifting material, as sand, powdered coke, etc., we can readily see how the bacteria can penetrate; but, with a solid wall, why is their transit barred at first and afterwards permitted. But one explanation is, I think, possible, namely that the holes of the material are in many cases larger than the diameter of the germs, and the germs are prevented from going through, not by being stopped on the outside, but by being caught against the rough walls or in the winding of the passage, just as a log of wood on a stream 50 times its diameter will, if the stream is at all winding, soon run against shore and stick. But this cannot go on indefinitely. Soon the angles are filled in with germ life, the slime of the zooglea formation renders the walls more or less smooth and slippery, and when a germ runs against the wall and is itself stopped it may as likely detach another, previously stranded,

which continues the journey. The difference in filtering power of a clean and uncleaned filter is thus easily explained. While with proper care such results as detailed above are easily obtainable, I would rather trust to unfiltered water than to the best filter systematically neglected. Since all by neglect accumulate organic debris, which acts as a breeding ground for germs, which can afterwards pass through in the manner outlined above; which word of caution will not I trust deter any from using a suitable filter, but simply emphasize the fact that when one is used common sense requires that it should be kept in working order.

ON THE SOLUBILITY OF ALUMINA RESIDUES FROM BAKING POWDER IN GASTRIC JUICE.

By Lucius Pitkin, Ph. B.

During the month of December, 1886, I was called upon to make some experiments to determine the solubility of the alumina residues, left in bread or biscuit after the use of an alum baking powder, in the process of digestion. This paper is a resume of my experiments upon two of the factors in digestive action, namely, the saliva and the gastric juice. It does not pretend to give any figure as to complete digestive action; it is, however, I think, reasonably accurate in regard to stomachic digestion, which is an important factor (probably the most important) in the whole process.

The special class of alum powders to which my experiments directly refer are those in which acid phosphate of lime is used, in connection with burnt alum, to liberate the carbonic acid of the bicarbonate of soda. It is to this class that, as I am informed, the majority of the alum powders of to-day belong; but the experiments while made with such a powder may also throw some light on other alum powders leaving similar residues.

The proportions used by me in making the powder to be tested were as follows:

Acid sodium carbonate (bicarbonate)	29 parts.
Burnt ammonia alum	20 "
Acid calcium phosphate	20 "
Corn starch	65 "

A glance at the above is sufficient to stop any discussion as to the existence of alum, as such, in the finished bread. The real question at issue in the use of such a powder is twofold.

1st. What proportion of the alumina residue left in bread after the use of the above powder is dissolved in the process of digestion? and

2d. What will be the effect upon health of the introduction of dissolved alumina in the quantity indicated?

It is obvious that the answer to the second question is quantitatively dependent upon the result obtained in investigating the first; and that while the second query belongs to the province of the physician and will be but incidentally alluded to here, the physician himself needs an answer to the question of solubility as a basis for his opinion.

The investigation, of which we here give the results, is not presented as showing the complete action of digestion upon the alumina residues. The difficulty of procuring pancreatic and intestinal juices effectually preclude the experimental testing (under the conditions of digestion) of their solvent powers in the case before us, since on account of the small percentage of alumina present large quantities of bread must necessarily be employed.

If we consider, however, the immense flow of the gastric juice in comparison with the other secretions of the alimentary canal and its markedly acid character, we must conclude, in the absence of all evidence to the contrary, that we are dealing with by far the most important factor of digestion so far as regards our problem. The gastric juice was obtained in the usual manner from dogs by means of a gastric fistula, and after complete recovery and a preliminary fast, the dogs were fed with fragments of lean meat, the outer surface of which had been hardened in boiling water. As the gastric juice was secreted it was allowed to escape by the fistula tube, filtered and bottled. But from one-half to three-quarters of an ounce was collected at once to insure freedom from digestive

products. The amount of gastric juice secreted per twenty-four hours reaches, according to the figures given in Flint's Physiology, no less than 14 lbs. per day. This large amount is no doubt due to resecretion following absorbtion, and is secreted for the most part during the ingestion and stomachic digestion of food. No figures can be obtained for the precise amount present during digestion, but assuming that 10 lbs. of the juice (of the total 14) is secreted during and immediately following the meals, and that the weight of the food is about 3 lbs. per day, we would have present in the stomach $3\frac{1}{3}$ lbs. of the juice to each pound of food. The experiments of Dr. Beaumont in the case of Alexis St. Martin have furnished us $3\frac{1}{2}$ hours as the time required for the stomachic digestion of bread. In the test then, for the solubility of the alumina residues in gastric juice, 60 grammes of biscuit were, after mastication, kept at a temperature of $37\frac{1}{2}$° C. (99° F.) for $3\frac{1}{2}$ hours with 200 grms. of gastric juice. The biscuits were prepared in the ordinary way, using two moderately heaped teaspoonfuls to a quart of flour, and the resulting biscuit weighed 818 grms., while the powder used weighed 13.615 grms. This amount of powder was found by analysis to contain 499.2 m.g. of alumina ($Al_2 O_3$), consequently, the 60 grammes of biscuit employed in the artificial digestion contained 36.6 m.g. of alumina, $Al_2 O_3$.

At the end of the three and one-half hours mentioned above, the mixture was filtered, thoroughly washed, and the alumina determined both in the residue and filtrate. The latter represents that portion of the alumina residue which is rendered soluble in the digestive process until such time as the food is discharged into the duodenum. The amount in solution was found to be 10.9 m.g. $Al_2 O_3$, while that remaining undissolved in the starchy residue was 23.2 milligrammes, a total of 34.1 milligrammes recovered of the 36.6 milligrammes calculated to be present. We can safely say then that under standard conditions of temperature and time, less than one-third of the alumina existing in the biscuit is dissolved in the nearest approach to stomachic digestion we can make. If alumina is judged to be harmful, then its injurious action must be predicated not for the amount of that element present in the powder, but for the amount rendered soluble and so capable of absorption and injurious effect.

In obtaining this factor of solubility of $\frac{1}{3}$ we would call attention to the fact that the investigation, if it has erred on either side, has been possibly too severe in the conditions of the experiment. All analyses tend to show that the gastric juice of the dog is stronger than that of the human being; the degree of acidity, on which especially the solubility of the hydrate and phosphate of alumina would depend, being much higher. The total solids in the gastric juice of a dog amount to more than 20 parts per 1,000, and while we would admit that the analysis of Bidder and Schmidt giving but 6 parts per 1,000 in human gastric juice was made on an abnormal secretion, we see no reason for doubting the figure determined by Berzelius, *i.e.*, twelve (12) parts. There is reason, therefore, for supposing that human gastric juice would have given us (could it have been procured) a smaller coefficient of solubility than the figure reported.

In brief, the results obtained for the solubility of alumina residues during the process of mastication and stomachic digestion (and leaving aside the action of intestinal juices), is somewhat less than $\frac{1}{3}$ of the alumina present when canine gastric juice is used.

This represents, we think, a maximum figure. From a comparison of published analyses human gastric juice is probably about one-half as strong. If solubility is approximately proportional to strength, then our figure should be changed to $\frac{1}{5}$ or $\frac{1}{6}$ for the solubility in mouth and stomach of man.

From the impossibility of procuring human gastric juice at will, a more exact approximation is probably not now possible.

"WATER" RESULTS AND THE PUBLIC.

By Prof. William P. Mason.

In the *Analyst* for April, 1883, Dr. Dupré writes an excellent article on "standards" for water analysis, in which he states that the laying down of a general standard is impossible. He also adds:

"This difficulty as to standards is certainly by no means confined to water analysis, but comes up whenever a standard is laid down for a natural product liable to variation."

Now my experience is that one is more frequently annoyed by a demand for "standards" in water analysis—more liable to be called upon to explain, translate and re-explain his results in that line of investigation than in any other.

Water analysts are treated very unfairly. There is an irritability manifested over their results usually unknown in other fields of analytical chemistry; the hasty critics forgetting, or being ignorant of, the fact that "water analysis," so called, is really less of an analysis than a series of experiments made with a view to assist the investigator in formulating his opinion as to the purity of the sample.

The analyst's "*opinion*" is the thing, after all, and yet how many water boards in the country would rest satisfied with that, and that alone.

The question that forces itself on my mind is, whether or not a water analyst pledges himself to educate his patron to the point of understanding analytical methods. Possibly it may be urged that the latter, without attempting to criticize the results himself, might, in his desire for an unprejudiced opinion, hand them to another chemist for his judgment. But even then there might be chance for misinterpretation; for how could the second chemist know the smaller details of the analysis, for instance the rate at which the ammonias have come over in Wanklyn's process, and upon which, as we all know, so much depends. How can he be aware of the dozen or more points, the noting of which goes in with the factors that make up the total "opinion," all of which are important and none of which appear upon the "report"?

Water analysts, again, are often called upon to report upon waters the history of which they do not know, and to base their opinions upon the analyses alone.

Such a demand is manifestly unfair. Recently a water was sent me for analysis which, upon being submitted to distillation for free ammonia, gave the enormous quantity of 1.18 parts per million.

What chemist would regard such a water as pure? And yet pure it really was. I found, upon inquiry, that it had been obtained by melting a cake of pure artificial ice. The melting had been conducted in an atmosphere more or less charged with the ammonia

used in the process, and the ice had taken up ammonia as a sponge would absorb water.

Again, I condemned a water recently which ran moderately high in nitrates, because of the sloping nature of the ground leading from a large cow yard towards the spring whence the sample was drawn. Had I not seen the ground I should have reported the water as wholesome. Subsequent investigation proved the correctness of the opinion.

Except in the instances of an exceedingly good or an exceedingly bad water, no one can safely venture an opinion as to quality unless he be admitted to all the facts in the case, and it is utter folly for the civil engineer to fail to furnish them as so frequently happens. Of course the ground taken is, that if observation be all, the engineer can furnish that himself; but observation is not all, nor nearly all, and yet the chemical side of the question cannot, as yet, get along without it. Where the necessary information is withheld I can see but one way open to the chemist, and that is for him to furnish the bare analysis, with full notes, and let the engineer make his own interpretation.

I have been so annoyed by being obliged to explain and re-explain water results, that I am seriously thinking of eliminating figures from my reports altogether, and writing the opinion only, or else attaching a printed slip, reading somewhat as follows:

"The writer of this report has arrived at the opinion herein expressed, after a most careful analysis of the water in question, and after having thoroughly weighed all evidence bearing in any way upon the result. He regrets that he cannot undertake to explain to anyone but a chemist his reasons for arriving at his conclusions."

RENSSELAER POLYTECHNIC INSTITUTE,
TROY, N. Y., Feb. 16, 1887.

ABSTRACTS.

GENERAL AND INORGANIC CHEMISTRY.

New Methods for Preparation of Crystallized Carbonates. M. L. BOURGEOIS.

The process consists in the precipitation by ammonium carbonate of the superheated solutions.

One method consists in taking an ammoniacal salt with a carbonate in suspension; the mixture is heated above 100° in a closed vessel. The author operates at 150°–180° on ½ grm. of amorphous carbonate and employs 2 grm. of ammoniacal salt, in the presence of 20 c.c. of water. After four or five heatings, followed by slow cooling, the total crystallization is obtained.

The other method is to precipitate slowly, but at once, the solutions by nascent ammonium carbonate, produced by the heating of urea in presence of the salt to be decomposed. The author in these experiments obtained no results with the carbonates of lithia, magnesia, zinc, manganese, iron, nickel, cobalt, or copper. (*Bul. Soc. Chim.*, **47**, *81.*) M. L.

Iodometric Studies. G. TOPF.

A very thorough monograph on the chemistry of iodine in relation to oxidizing and reducing agents. (*Zeit. anal. Chem.*, **26**, 138–217.) J. F. G.

ORGANIC CHEMISTRY.

On Fahlberg's Azoto-sulphureted Saccharine. E. MAUMENÉ.

The conclusion of the author is that saccharine as produced by Fahlberg is not a defined species. It is composed of at least two parts.

2. It does not by itself nor by its components answer to Fahlberg's formula.

3. It seems to be formed of two parts or bodies of equal weight. (*Bul. Soc. Chim.*, **47**, *92.*) M. L.

Action of Ethyl Aldehyde on Resorcine. H. CAUSSE.

The author dissolves 50 grms. of resorcine in 500 c. c. of H_2SO_4 at 10%, and puts on the water bath. Every 5 minutes a portion of 5 c.c. of a solution of aldehyde is added. This solution contains 1 grm. of pure aldehyde to 9 grms. H_2SO_4 of 10%. The author insists on the precautions essential to a good result; the crystals obtained had the composition:

C	67.80
H	5.71
O	26.49

The analysis leads to the formula $C^{14}H^{14}O^4$, or 1 molecule of aldehyde, 2 mols. of resorcine, minus 1 mol. of water. (*Bul. Soc. Chim.*, **47**, *90.*) M. L.

ANALYTICAL CHEMISTRY.

Use of Bromine for Decomposing Sulphides. A. BRAND.

"*Bromum solidificatum*," which is *kieselguhr* saturated with bromine, and so prepared that a stick 1 cm. in length and 7 mm. diam., contains about 1 grm. bromine, is a convenient substitute for chlorine where the latter is generally used for decomposing minerals, slags, etc. The pulverized mineral or sulphide is placed in a heavy glass tube, connected with two or more U tubes containing hydrochloric acid, which serves to intercept the bromides and bromine. A suitable quantity of the bromine sticks is then introduced into the tube, followed by a plug of stiff gypsum paste, and the end of the tube is tightly corked. Heat is then gradually applied to the tube and the bromine made to pass over the pulverized mineral, and the latter finally heated when the bromine is all expelled from the sticks. The volatile bromines are thus expelled and intercepted in the hydrochloric acid, which, as soon as the operation is completed, is emptied into an evaporating dish, the bromine expelled by evaporation, and the analysis further conducted as if chlorine had been used for the decomposition. (*Zeit. anal. Chem.*, **26**, 222–226.) J. F. G.

Determination of the Sulphides of Zinc and Cadmium. P. VON BERG.

These sulphides can be rapidly determined by iodine solution, by acting upon the freshly precipitated sulphides with a known quantity of iodine solution containing a small percentage of HCl. The reaction being $ZnS + 2\,HCl + 2\,I = ZnCl^2 + 3HI + S$. (*Zeit. Anal. Chem.*, **26**, *p. 23.*) J. F. G.

Analysis of Clay. MEINEKE.

Determination of silica: The author shows the necessity of carefully testing the filtrates after the first separation of the silica, as the loss of silica by solution is apparently greater than the error generally caused by contamination of the separated silicic acid by alumina. (*Rep. anal. Chem.*, **7**, 214.) J. F. G.

Volumetric Determination of Zinc Powder (Slate Gray of the Vieille Montagne). F. WEIL.

The process consists in treating a certain amount of the zinc powder with an excess of a neutralized copper salt, of a determined strength; the zinc precipitates copper equivalent for equivalent. The non-precipitated copper is afterwards titrated by a solution of stannous chloride; the difference, with the amount of copper taken, indicates the copper precipitated by the zinc. This number, multiplied by the coefficient 1.0236, gives the metallic zinc of the sample. The author indicates the *modus operandi* of his method. (*Bul. Soc. Chim.*, **47**, *83.*) M. L.

New Method for the Determination of Starch and Different Kinds of Sugars. J. EFFRONT.

Soxhlet has shown that the methods actually used for determination of starch are not accurate, sometimes giving rise to errors of 1 and 2%. The author has found a method of determination of starch and dextrine in presence of other sugars. The liquid to be tested is treated in succession with NH_3, sodium hypochlorite and HCl. The treatment with NH_3 destroys inverted sugar, and a portion of glucose and maltose. The hypochlorite completes the destruction. The liquid is diluted to 4–9% sugar, and its rotary power is observed. 10 c.c. are mixed with 10 c.c. of NH_3 at 22° B and 5 c.c. of water; the liquid is kept in the water-bath for 40 minutes and then concentrated to 5–8 c.c. The concentrated liquid is put into a vessel cooled by water, and treated with 10 c.c. of sodium hypochlorite (7–8% active Cl). The liquid is stirred and treated with 2 c.c. of concentrated HCl and diluted to 25 c.c. The rotary power of this liquid is observed; the rotation is produced by dextrine. For the determination of starch the author uses Dubrunfant's method, modified by him, converting the starch into dextrose and maltose and observing the rotary power before and after destruction of the sugar by the author's process. (*Bul. Soc. Chim.*, **47**, 7.) M. L.

Detection of Small Quantities of Albumen. R. PALM.

The delicacy of the reactions for albumen with acids can be much increased if the reagents are applied in solution of 95% alcohol, or still better, in alchol containing 10% of ether. This also prevents the redissolving of the precipitates in excess of the reagent. The following reagents are specially for the precipitation of very minute quantities of albumen :

1. Alcoholic solution of basic ferric acetate completely precipitates the albumen on gently warming.

2. Alcoholic Solution of basic cupric acetate.—The precipitate caused by the same is dissolved in acetic or lactic acid, and excess of NaOH added, and the solution then heated to boiling, when reduction occurs.

3. Solution of lead acetate or chloride in alcohol.—The precipitated albumen can be further tested with glacial acetic and sulphuric acids, whereby a violet coloration should result (Adamkiewicz's test).

4. Traces of albumen, also precipitated by lead hydrate.—The latter freshly precipitated is dissolved in boiling water, and this solution used for the test. (*Zeit. Anal. Chem.*, **26**, *p. 35-38.*)

J. F. G.

INDUSTRIAL CHEMISTRY.

Regeneration of the Acid Residues from the Manufacture of Gun Cotton. E. ALLARY.

1. SIMPLE DISTILLATION.—The acid residue indicating 58° B^e. was filtered through quartz. 100 kilos gave—

10.077 kilos Nitric acid at 50° B^e.
6.279 " " " at 10° B^e.
83.302 " Sulphuric acid at 62° B^e.

2. DISTILLATION WITH SODIUM NITRATE.—The author uses these residues instead of sulphuric acid in the decomposition of sodium nitrate. He obtains at once acids at 48°45, and even 49°40, by using dried nitre.

No danger is to be feared if the residual acids are previously filtered. (*Bul. Soc. Chim.*, **47**, *102.*) M. L.

Battery with Elements of Coal without Metals. D. TOMMASI and RADIGUET.

The positive electrode is a stick of carbon covered with lead peroxide and placed in a linen bag. The whole is placed in a cylinder of carbon, perforated, and the two elements are put in a jar filled with pieces of gas retort carbon, and covered with a solution of salt. The electromotive power is 0.6 volt. to 0.7 volt.; it is good only for short operations, but for those cases will last for years. (*Bul. Soc. Chim.*, **47**, *83.*) M. L.

Adulteration of Split Peas. A. KLINGER and A. BUJARD.

In the laboratory at Stuttgart peas were received which were found to be colored with fuschin. A secret preparation recently sold in the vicinity of Stuttgart under the name of "Dr. S. S. Louden's Chemical" for improving petroleum was found to be simple sodium bicarbonate. (*Rep. anal. Chem.*, **7**, 232.)

J. F. G.

Abstracts of American Patents Relating to Chemistry.

(From the Official Bulletin of the U. S. Patent Office.)

February 8th, 1887.

357,227.—Lubricating composition. J. Plante.

Consists of powdered sulphur, peat and oil.

357,259.—Disinfectant mixture. E. G. Xander.

One part of ferrous sulphate and four parts of coal tar are intimately mixed with forty parts of charcoal.

357,273.—Blue coloring matter from tetrazodiphenyl. C. Duisberg.

Is produced by the action of tetrazodiphenoldimethyl ether upon alpha naphthol.

357,244.—Red coloring matter for dyeing by the action of tetrazo dyes with betanaphthylamine sulpho acid. C. Duisberg.

357,281.—Dyeing with basic aniline. E. Holliday and E. Rau.

The material is dyed in a bath composed of the fatty salts of the basic coal tar colors in a solution of benzine, etc., and then steamed.

357,331.—Process of obtaining albumen from blood. T. Nordenfeld.

Consists in separating, by stirring or whisking, the fibrin from blood, then adding to the serum sugar and paraffin oil, afterward separating the particles by centrifugual action, and finally evaporating and drying the albumen.

357,465.—Process of preparing powdered milk. J. Carnrick.

Consists in evaporating the fresh milk at a low temperature in vacuo to a syrupy consistence, and then, while subjected to continual agitation, adding one or more of the different kinds of sugar, and continuing the evaporation and agitation until brought to a condition of dry granular powder.

357,528.—Process of concentrating sulphuric acid. J. B. F. Herreshoff.

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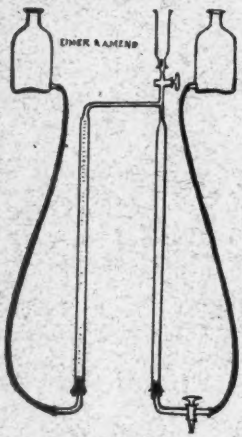

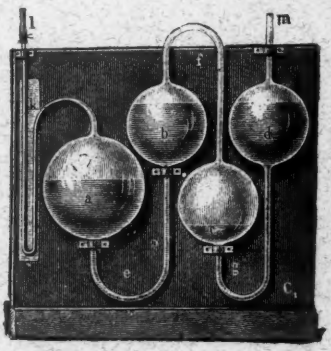

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